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## The Not-So-Secret Life of Plants

*In which the historical and experimental myths about emotional communication between animal and vegetable are put to rest*

In the troubled years of the late 1960s, a wave of antiintellectualism swept through the United States, accompanied by an antisecularism that still persists in some measure. Some public antipathy to the methods and products of science was understandable, because certain of the technological applications of science had failed to better man's condition and indeed had perceptibly diminished the pleasure and grace of modern existence. Critics were quick to equate science with antihumanism, and to call for reliance on alternate ways of arriving at an understanding of the universe about us. This appeal found receptive ears in a world worried about pollution, overpopulation, unemployment, growing crime, and—perhaps most important—a nasty and persistent war in which technology played a major role.

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Onto this scene, in 1973, burst a book, *The Secret Life of Plants* (1), which claimed for members of the vegetable kingdom many mental capabilities previously regarded as limited to gods, human beings, and some higher animals. These included the ability to perceive and respond to human thoughts and emotions and to distant traumatic events, such as the injury or death of other organisms. Quoting from uncontrolled experiments, random observations, and anecdotal reports, the book fashioned a case for the ability of plants to count, to communicate with each other, and to receive signals from life forms elsewhere in the universe. Plants were alleged to respond favorably to certain forms of music (e.g. preferring Bach to rock); to display conditioned reflexes; to predict storms, earthquakes, and the like; and even to transmute elements (in order to avoid mineral starvation). Among the many bizarre claims, the one that strains credibility the most is the assertion that we can rid plants of insect pests, or fertilize the soil in which they grow, simply by exposing *photographs* of the growing plants to particular frequencies of electromagnetic radiation. Throughout, the book indiscriminately mixed accounts of generally accepted phenomena with unsubstantiated and incredible reports.

The authors of the book, Peter Tompkins and Christopher Bird, are, without question, adept popularizers of scientific and technological topics and are certainly acquainted with some aspects of modern plant research. Moreover, the issuance of the book was shrewdly timed to take advantage of the general malaise about science noted above. These facts, plus a lavish advertising campaign and

several book-club selections, made the book vastly popular. (Perhaps the ultimate measure of its success was the spate of cartoons it inspired in the *New Yorker* and in the syndicated strip *Doonesbury*.) Had the majority of readers taken it in with the joy of escape to fantasy that may be accorded a good novel, no damage would have been done. This was not what happened, however. The book catalyzed numerous claims of bizarre observations from "one-man" laboratories; it led to a widespread lay criticism of professional scientists for not taking account of the purported facts of plant existence; and it permeated student arguments in university biology classes.

In response to this uncritical acclaim, though perhaps after undue delay, several scientific reviews of the book appeared (2, 3, 4), and the American Society of Plant Physiologists (ASPP) and the American Association for the Advancement of Science (AAAS) scheduled sessions to evaluate some of the claims made. One of the most tangible and also crucial portions of Tompkins and Bird's book is a discussion of electrophysiological experiments on plants conducted by polygraph expert Cleve Backster (5). In June 1974, at the ASPP meeting, Dr. B. G. Pickard of Washington University organized a symposium at which independent and well-controlled experiments were described (6) that had attempted—but failed—to reproduce Backster's results. Following this, Galston organized a session at the AAAS meeting in January 1975, which brought Backster *face to face* with some of his critics, including the two scientists, E. L. Gasteiger and J. M. Kmetz, who had gone to great lengths to reproduce Backster's experimental condi-

tions. Subsequent to this meeting, a number of national television and radio confrontations were organized, but these eventually ceased when scientist participants realized that the hoped-for dialogue was replaced by unbending restatements of previous positions.

Since it is on the interpretation of electrophysiological data that Backster's case and much of the Tompkins-Bird case rests, the purpose of the present essay will be to reexamine the published experiments and to relate them—from the point of view of both philosophy and technical procedures—to the body of controlled and reproducible electrophysiological experiments that have been carried out on plants.

## Electrophysiology of plants

It is commonplace knowledge, reaching back to Galvani (7) in the eighteenth century, that variations of electric potential are an essential feature in the function of specialized animal cells and tissues, manifest most conspicuously in the transient electric signals, called action potentials, found in nerves and muscles after stimulation. Judging from the lay response to the Tompkins-Bird book, however, and from the queries of freshman biology students, knowledge of related electrical phenomena in plant tissues has been restricted almost entirely to professional scientists.

The impression left by the book, and apparently accepted by a considerable portion of its readers, is that the only important early work in plant electrophysiology was that of the eminent Indian physicist J. C. Bose and that that work was ignored for forty to fifty years, until Backster's report was published in 1968. We shall deal later on with the substance of Backster's experiments, but we need first to dispel this mistaken historical impression and to develop some fundamental ideas in the modern scientific view of "plant electricity."

In retrospect, Bose certainly does deserve great credit. With vast ingenuity, he designed sensitive and elegant instruments to measure small changes in electric potential and small changes in shape (bending,

swelling, etc.) of plant materials. With these instruments he was able to demonstrate withdrawal movements of plant tissues from sites of injury, complementing for "ordinary" plants the much more conspicuous movements displayed by various insectivorous and photoperiodically sensitive plants (e.g. Venus's-flytrap, sundew, and *Mimosa*). He also described events in aquatic higher plants that resembled action potentials (8), while other investigators, following the lead of Burdon-Sanderson in 1873 (9), demonstrated action potentials (and in some cases simple integration) in insectivorous plants, mechanosensitive plants, and giant algae (10, 11). Since Bose's time, events similar to action potentials have been described in a wide variety of plant cells (12) and even in fungi (13). Bose quite properly pointed out *functional* similarities between the electrical/mechanical responsiveness, or irritability, of plant and animal tissues, but his data do not in any way support Tompkins and Bird's conclusion that plants perceive their environment in the manner of human beings and other higher animals.

While Bose's thought was very advanced in some respects, it was rather primitive in others. His American contemporaries, chiefly the school of W. J. V. Osterhout, were far closer to a proper physical understanding of electrical events in plants. Drawing on the emergent theory of electrolytic solutions set forth by Nernst, Planck, and other illustrious physical chemists of the late nineteenth and early twentieth centuries, Osterhout (11) reasoned that differences of electric potential between the interior of (algal) cells and the external environment must result from differential diffusion of ions (e.g. of sodium, potassium, and chloride) through selective membranes at the cell surface. This was the first major component of the modern theory of bioelectricity. Osterhout even suggested that the selectivity of cell surfaces might arise from the existence of carrier molecules residing within the surface, a suggestion that presaged much of modern cell biochemistry (14).

The other major component of the modern theory began to emerge in the 1930s, from experiments carried out by E. J. Lund at the University of Texas, by H. S. Burr at Yale, and by H. Lundegaardh in Sweden. Lund

observed that differences of electric potential measured along plant stems and roots (we shall return to the methodology of these measurements below) were acutely sensitive to metabolic poisons (15), while Lundegaardh found that the same differences of potential, along with the rate of oxygen consumption by the tissues, were closely tied to cellular uptake of anions from the medium (16). The inference was drawn that metabolism could "pump" charges, both ionic and electronic, through the cell surface membranes, thus creating an electric-potential difference across the membranes. This idea came to fruition in the late 1960s, after P. Mitchell (17) realized that it could account for numerous experimental data on the movement of ions and trapping of energy by mitochondria and chloroplasts, the subcellular organelles responsible for conserving energy from the oxidation of sugar and from the capture of light, respectively.

The primary charge separation giving rise to bioelectric potentials takes place across the bounding surface membranes of individual living cells, whence comes the familiar piece of jargon "membrane potential." Widespread verification of this fact was delayed until the late 1940s and 1950s, following the development of glass microcapillary electrodes that could be inserted into individual living cells. Osterhout's work used a macroversion of this insertion technique, taking advantage of giant algal cells, while the experiments of Bose, Lund, Lundegaardh, and their other contemporaries (in both plant and animal electrophysiology) relied upon whole-tissue recording techniques.

Typically, pairs of saline-bathed wick, wire, or capillary-suction electrodes were used, one member of the pair being placed at an "interesting" spot on the tissue, and the other member at a distant—and presumably "indifferent"—spot. The most important and familiar example of this technique is the recording of electrocardiograms via a skin electrode placed over the chest wall and another electrode, for comparison, fastened to an arm or leg. The technique is very much simpler than the intracellular recording technique, but it is also much more treacherous, since it depends on many diverse features of the whole recording circuit: the

quality of the electrode-tissue contact, the size of the electrodes relative to the size of individual cells, the presence of "dead" space (cuticles, connective tissue), the constancy of potential near the indifferent electrode, the relative magnitudes of electric resistance *between* cells as compared to *around* cells in the tissue. Thus, whereas cell membrane potentials vary only in the range of 10 to 300 millivolts, extracellular recorded potentials range from a few microvolts (as, for example, in electroencephalograms) to hundreds of volts (across certain fish electroplates, 18).

In both plant and animal electrophysiology, interpretation of data from even the most careful and technically sophisticated measurements has often been vitiated by elementary biological mistakes; for example, cell damage during preparation of the experiment can produce prolonged depolarization, with or without semi-periodic fluctuations. Typically, such damage response displays a rapid—almost instantaneous—onset, but recovery can be delayed for periods of tens of minutes to several hours. Owing to the natural impatience of a scientist to "get on" with his work, this kind of artifact has permeated a very considerable fraction of published experiments.

Nevertheless, in careful hands, both intracellular recording techniques and whole-tissue recording techniques can yield much important information, the whole-tissue techniques being particularly suitable when scientific interest is focused on electrical gradients that extend long distances, over many cell diameters. Growing regions of plants provide an excellent terrain for such measurements. Thus, the growing tip of the seedling leaf sheath of oat plants was found by Lund (15) to be about 100 millivolts negative to the base of the organ, and similar electric gradients have been observed along other plant organs, including roots, stems, and reproductive axes (19).

Furthermore, these longitudinal gradients can be altered by mechanical distortion, and by changes in temperature, light, and ambient salt concentration (15, 20). While normally no transverse electrical gradient exists across cylindrical plant organs, certain stems, roots, and leaf

stalks can be polarized transversely, to 100 millivolts or more, by exposure to light or gravitational fields. And the polarization correlates with redistribution of the growth hormone, auxin, and subsequent growth curvature (21).

Electric currents accompanying growth changes in single fertilized eggs of marine algae, such as *Fucus* and *Pelvetia*, have also been measured, using ultrasensitive extracellular electrodes (22). Early electric polarization of eggs, whether spontaneous or induced by external gradients of light, temperature, etc., accurately defines the direction of growth and initial planes of division for the developing embryos. In this case calcium-ion migration, rather than hormone redistribution, is the chemical event most crucially linked to the electrical change (23).

It should also be noted that various rhythmic "clock" functions manifest in plant tissues are associated with changes in electric potential, which can readily be observed either with intracellular or extracellular electrodes. The best-known of these display roughly 24-hour (circadian) periods, phase-locked to the normal day-night cycle, but they can persist for long periods in the absence of light stimulus, and can also be shifted or reset by pulsed light absorbed by a specific pigment called phytochrome. As an example, diurnal leaf opening and closing in plants such as *Samanea* (24) involve periodic depolarization and repolarization of different groups of cells in the leaf motor organ. Although the precise causal relationships have not been worked out for these systems, it is evident that both the cellular swelling and shrinking that produce the leaf movements and the measured changes of electric potential arise from periodic cellular uptake and release of potassium chloride (25, 26).

## The Backster report

Cleve Backster, whose experiments in plant electrophysiology have been so handsomely reported in the Tompkins-Bird book, is a trained polygraph (lie-detector) specialist, whose introduction to the method came during service as an interrogation instructor in the U.S. Army Counterintelligence Corps; thence he traveled

to the Central Intelligence Agency, where he developed the standard polygraph examination methodology. From the C.I.A., he went on to become director of a polygraph institute in Chicago, and in 1951 he founded the Backster School of Lie Detection. He claims to have served as a consultant to almost every government agency that has used the polygraph and to have made numerous technical refinements, aimed at reducing the number of inconclusive examinations. Within his field he is well recognized, having served on the Board of Directors at the American Polygraph Association and as chairman of the research and scientific committee of the Academy for Scientific Interrogation.

When used on human subjects, the polygraph records uncontrolled reactions of the autonomic nervous system—chiefly sweating—which are often associated with lying and other emotional disturbances. It does this by measuring, via what is called a Wheatstone Bridge circuit, a fall of electrical resistance in the skin between two independent surface electrodes (wire mesh or plate, in saline/agar paste). The phenomenon is known as the psychogalvanic reflex.

Backster's initial foray into plant physiology (5) evidently came almost casually, as he wondered whether the surface resistance of plant leaves might be analogous to human skin resistance, and might reflect variations in the flow of sap following routine watering. It seemed a reasonable question, and he attached a polygraph to a common potted houseplant, *Dracaena*, in his office. Polygraph experience suggested that increased transpiration and water-flow through the leaf should *decrease* the transverse resistance of the leaf (the electrodes were attached to opposite leaf surfaces).

As is often the case in new experiments by any electrophysiologist, the initial records were rather noisy. They contained at least two unaccounted-for quasi-periodic variations, at about 1 cycle/second and 2 cycles/minute, superimposed on a baseline drift that suggested, if anything, a progressive *increase* of leaf resistance. There was also a segment (of the single record that Backster published) that superficially resembled the slow rise-and-fall of resistance that occurs in

human polygraph records during verified lying (Fig. 1).

At this point there took place a totally unscientific discontinuity of logic. Without investigating the recording conditions to identify the sources of unexpected noise and drift, Backster jumped to the conclusion that because the plant record resembled in a single respect human records obtained during emotional reaction, the plant must have been experiencing something like human emotion. This is a classical semantic confusion of identity, roughly equivalent to arguing that because the face of the full moon displays dark patches resembling a human face, there must be a real man in the moon.

Backster thereupon decided to explore further the possible emotional response of his plant by affronting it in ways that evoke strong emotional reactions in human beings—with injury or threats of injury and death. He reports finding that scalding a nearby leaf (with hot coffee!) was not sufficient to evoke a response on the polygraph, but that when he *thought* about burning the leaf with a match there was a “dramatic change in the tracing pattern” (Fig. 2).

Similar but undocumented experiments were subsequently carried out on other plant species, “frequently serving to reinforce” Backster’s hypothesis. No interpretation was given of those experiments which failed to reinforce the hypothesis. The author notes that the phenomenon “persisted when the plant leaf . . . was detached from the parent plant, and even when . . . shredded and redistributed between the electrode surfaces.” In view of the known effects of injury on electrical manifestations in tissue, the latter claim is most remarkable.

Thereafter, Backster refined the format of his experiments in several ways. First, he devised a constant and remote emotional stimulus in the form of scalding death of brine shrimp (*Artemia*). Second, he selected *Philodendron cordatum* as his recording species, its leaves being optimally stiff, broad, and thick. Third, he automated the experiments and ran them in triplicate. Fourth, he connected a recorder to a 100,000-ohm resistor, instead of a leaf, as a control against instrument noise. Fifth, he isolated the main recording instruments, each plant, and the brine shrimp in separate rooms of the

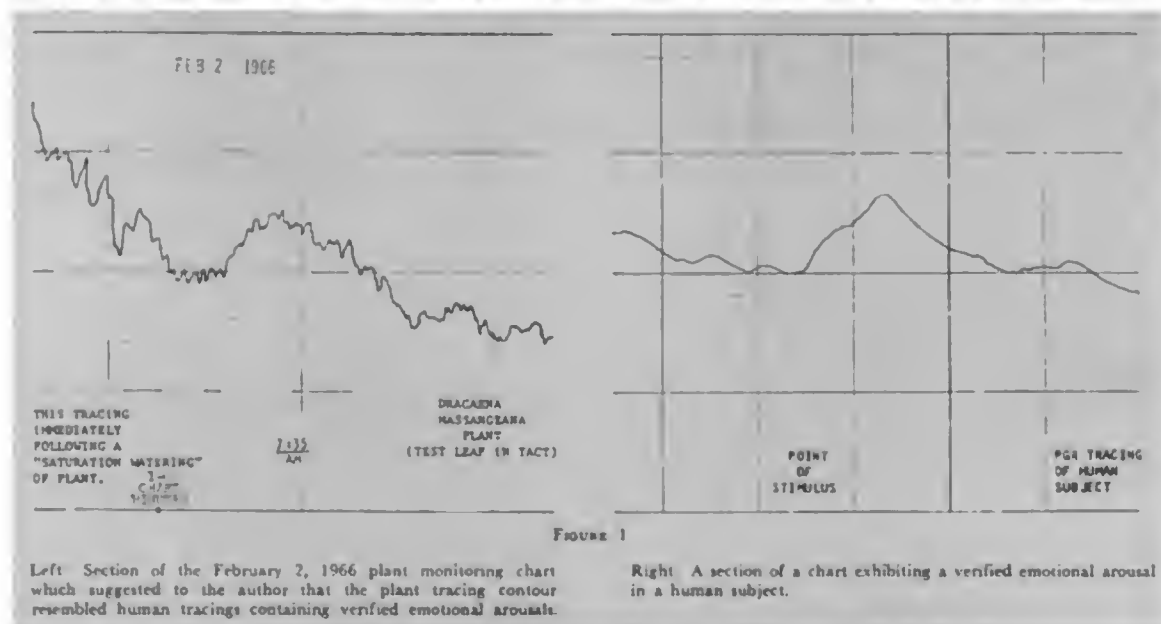
laboratory. Sixth, he used a blind randomizer, along with controls of sterile brine (no shrimp), so the experimental observer would not know when or whether, during a particular experiment, shrimp had actually been killed. And seventh, he kept light and temperature constant for all plants.

The analysis of data from seven experiments carried out in this manner is as follows. All seven of the fixed resistor tracings were flat, giving no indication of electronic disturbances in the instruments. Of the twenty-one leaf records, two were discarded because of failure in the pen recorders; three were discarded because of “gross overactivity”; and three were discarded for “not displaying typical fluidity.” This left a total of thirteen usable chart records. Each experimental run was 2.5 minutes long and was divided into 6 blocks of 25 seconds. During the first 10 seconds of each block a shrimp-killing could occur, but in only 13 blocks (designated stimulus blocks) out of the 78 total did a killing actually take place. The remaining 65 blocks were designated control blocks. A sudden deviation of leaf resistance was scored as a positive reaction, and this occurred in 11 of the 13 stimulus blocks, but in

Figure 1. A polygraph recording for the leaf of a houseplant and one for a human subject undergoing verified emotional arousal exhibit

somewhat similar contours. While this fact suggested to Cleve Backster, who performed the experiments, that plants experience emo-

tions, the plant record was probably made at a stage when the testing system was unstable and may therefore be invalid. (From ref. 5.)





only 8 of the 65 control blocks. No positive reactions were obtained in control runs with the sterile brine alone. Backster inferred that plant cells must have "a primary sensory system."

This is the sum total of published experimental information underlying the widely publicized claims. The report, appearing in the *International Journal of Parapsychology*, Winter 1968, stands today as the only report of such results, having apparently not been reproduced, even by Backster. It was supplemented, however, during the 1975 AAAS meeting. There Backster described new experiments, in which the recording organism was *Lactobacillus* (yogurt). Pots of yogurt were said to display changes of electric potential upon addition of oxidizable substrate to nearby pots. This, too, received extensive press coverage.

## Experimental rebuttal

When confronted with reports that are as weak and logically faulty as this, the professional scientist is tempted to dismiss them out of hand. Identifying and cataloging all of the uncontrolled experimental variables

is likely to be a very time-consuming task, and one that, because of probable negative conclusions, seems unrewarding. We are indebted, therefore, to two serious and dedicated men, E. L. Gasteiger, of Cornell University, and J. M. Kmetz, then of the Science Unlimited Research Foundation in San Antonio, Texas, who eschewed expediency in order to confirm or reject the Backster report from solid experimental ground. Verbal advice was given by Backster to both researchers, so that the experimental conditions and layout would be as nearly identical with the original circumstances as possible. Both sets of results have been presented to the public (in the 1975 AAAS symposium), and one has been published (6).

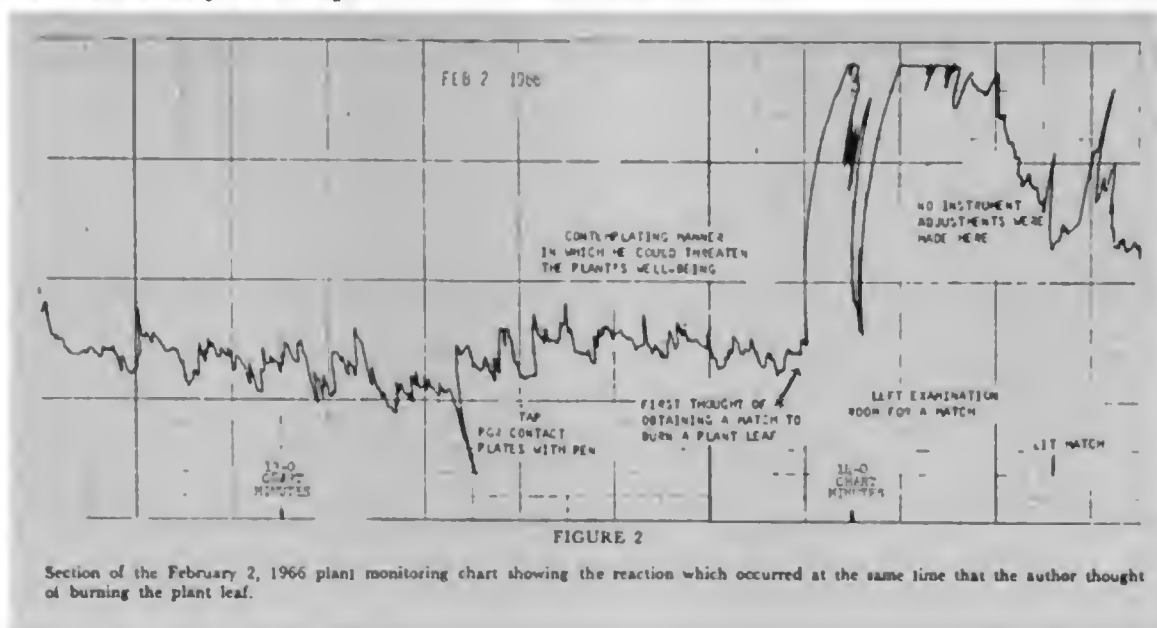
Gasteiger, working in collaboration with two undergraduate students, K. A. Horowitz and D. C. Lewis, arranged experiments with all seven features listed above in Backster's refined format. Some further improvements of the procedure were also incorporated, including maintenance of the plants in light-tight rooms; ejection of the brine shrimp into the boiling water by solenoid-driven pipettes, whose electrical ac-

tivating pulse was automatically recorded; videotape monitoring of the entire shrimp-killing operation; careful insulation and shielding of the electrodes, plant preparation, and electric cables; and use of high-gain, capacity-coupled voltage amplifiers to measure microvolt changes of potential, rather than resistance, across the leaves. The latter modification has substantial advantages, particularly in its greater sensitivity and in its lesser vulnerability to extrinsic noise.

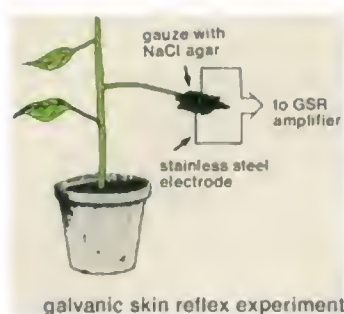
As befits the extreme care used in arrangement of the electrophysiological apparatus, the resultant records were much "quieter" than those of Backster and displayed only one feature that could possibly have been construed as a response to the emotive stimulus. That feature was a brief 10- to 45-microvolt deflection which occurred spontaneously and at irregular intervals, in about 30% of the plants studied. Horowitz, Lewis, and Gasteiger scored the deflections as positive, negative, or null, depending on whether the amplitude of the one immediately following a shrimp-killing (or trial with water alone) was larger than, smaller than, or equal to the one (if any) occurring in the im-

Figure 2. This recording suggested to Backster that plants can respond to human thoughts. No methodology has been given for timing the

onset of a thought, but the record certainly does become erratic. (From ref. 5.)

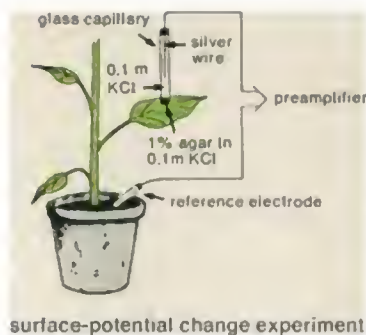


Section of the February 2, 1966 plant monitoring chart showing the reaction which occurred at the same time that the author thought of burning the plant leaf.



galvanic skin reflex experiment

Figure 3. Backster's setups for monitoring galvanic skin reflex (left) and surface potential changes (right) in plants were reproduced and somewhat refined by J. M. Kmetz. In both



surface-potential change experiment

setups the concentration of sodium chloride in the agar gel took 20–30 minutes to stabilize; Backster's records were probably made before this point was reached. (From ref. 27.)

mediately preceding control period. A total of 60 randomly sequenced shrimp-killings and 40 water trials were conducted with 20 plants. Statistical analyses of the data were carried out in several different ways. Under no identifiable criterion could either the shrimp-killings or the water trials be associated with voltage shifts.

To be sure, the authors did find Backster's reported data to be significant by the same kinds of statistical analyses; but, they stated, "we matched, and in several instances improved on, Backster's experimental techniques. . . . We obtained no evidence of primary perception in plants. While the hypothesis will remain as an intriguing speculation, one should note that only the limited published data of Backster support it."

Kmetz (27) was even more assiduous than the Cornell group in replicating Backster's experimental conditions, and in particular, he returned to the Wheatstone Bridge method of recording leaf resistance (Fig. 3). But after 168 trials on 42 plants, involving 84 shrimp killings and an equal number of water trials (Fig. 4), he could find no significant correlation between resistance shifts and shrimp killing. He also delved into the "yogurt experiment" with the same thoroughness—and the same negative result.

Perhaps more important, Kmetz carried out the time-consuming and difficult task of ferreting out and

documenting, from a complicated experimental protocol, the probable source of Backster's results. We have already noted that Backster's chart records from plants were noisy, much noisier than the polygraph record he produced from a human subject. When viewing, for example, Figure 2, from the 1968 article, anyone experienced with practical electric circuits (and particularly anyone experienced with bioelectric recording) would suspect an unstable electrical junction somewhere; and even for the much cleaner record of Backster's fifth figure (not shown here), obtained from his refined experimental format, the low-level irregularity of the trace and the baseline blurs (mechanically attenuated higher-frequency oscillations?) raise suspicions. The problem was to identify the exact point of the bad junction.

Kmetz had noted that the plant records appeared "more active" immediately after connecting the leaves to the recording system than they did later on, and so he carried out one novel and crucial control measurement: instead of recording simply from a 100,000-ohm resistor as the control against instrumentational noise, he included a pair of the Backster electrodes (still without a leaf) in that circuit. The resultant records were every bit as "active" as when a leaf was included, and they calmed down with time.

Kmetz studied the electrodes visually in many experiments and found that immediately after placing a set of electrodes in operation, water begins

to evaporate from the agar gel, causing a change in concentration of sodium chloride in the gel. This change in turn causes a change in resistance, which appears as a pen deflection on the recorder. Since the evaporation process is not uniform, rather wide variations in resistance occur, and the system is thus extremely unstable just after it is set up.

After a period of time, a "skin" forms between the agar and the air. Although the skin does not completely prevent the drying process, it retards it sufficiently to make the electrode system appear stable. Kmetz's observations indicate that the equilibration time is 20–30 minutes. From the description given in his 1968 paper, it appears that most of Backster's plant readings were taken during the equilibration period. Kmetz suggested that any readings taken then might be invalid.

## Sensory reception

It thus appears clear that Backster's experiments do not support either his hypothesis or the Tompkins-Bird claims. Undoubtedly most of the other experiments purported to reflect humanlike emotional or sensory behavior in plants could be debunked with the same precision. The cost, however, would be unacceptably high in both time and scientific distraction.

For this reason, and because the notion of emotional plants admittedly does have revolutionary implications, the idea will surely float around on the fringe of science for a long time to come. And, to be sure, both the logic and the history of science require us to be alert to the possibility—however remote—that harder data may one day turn up. It therefore seems appropriate to discuss briefly what identifiable features a bona fide sensory-communication process in plants might possess and how an experimenter might go about demonstrating the presence of such a process. We address the problem only for small-signal processes. Large signals are, by definition, easy to handle.

All biological receptor phenomena that we know of, and certainly all that manifest themselves in electrical signals, can be described by five parameters which should be relatively constant in any given experimental



circumstance: waveform (shape), amplitude, duration, latency, and—often most important—noise. The noise can play very nasty tricks, either by generating pseudophenomena (as implied by Kmetz's study) or by hiding real phenomena. These dangers exist even when intracellular electrodes are used to record from individual cells, and they become a dominant problem in extracellular, massed recording. In many cases, experiments must be specifically designed to use presumed latency, amplitude, or duration parameters in order to extract real signals from background noise.

Consider, for example, that most animal sensory neurons discharge signals (action potentials) spontaneously, usually rather slowly—at rates on the order of a few per second—without any known changes in their environment. On a time scale of tens of seconds these spontaneous discharges appear to be distributed randomly, with some average frequency, and some standard error (a measure of variability), which can easily be as large as the average frequency. If a particular fiber in the mammalian optic nerve discharged an average of 3 times per second, then over a period of time it would display 1-sec intervals with any number of action potentials between zero and, say, 10. Some intervals would have 3 discharges, but most would not; many would have none, and an occasional interval would contain 8–10.

The question, now, is how an experimenter can detect a response to a low-level optic stimulus, one sufficient to increase the discharge by, say, 1 impulse/sec, on the average. There is no way other than to examine the neuronal discharge after numerous repeated stimuli. In fortunate circumstances, discharges due to the stimulus would *tend* to appear at a fixed interval—say 10 milliseconds—following the beginning of a stimulus, such as the turning on of a light, regardless of the frequency or timing of the background discharge. Reproducibility in this interval of latency is a very important criterion by which to identify a causal relationship between retinal illumination and electrical discharge within the optic nerve. If reproducibility fails, then one must resort to long-term statistical analysis of the neuronal discharge.

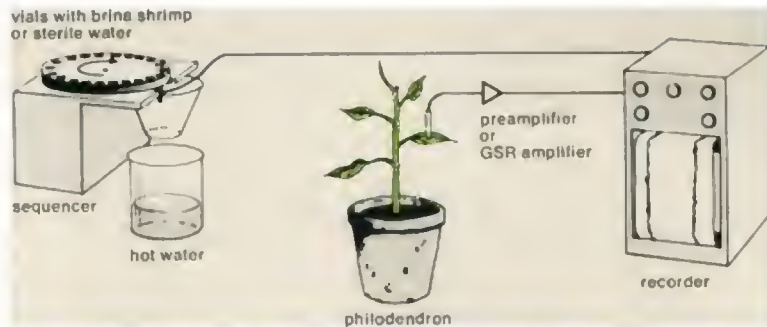


Figure 4. Kmetz reproduced Backster's experiment on the effects of brine shrimp-killing on electrical responses in plants. Randomly sequenced shrimp-killings and sterile-water control tests were correlated through a recorder with changes in leaf resistance in a houseplant.

Backster reports having found a positive correlation in 11 out of 13 cases where a killing actually occurred and in only 8 out of 65 cases where no killing took place, but Kmetz could not reproduce Backster's results. (From ref. 27.)

A related procedure is often used in massed-cell measurements, as in studying brain signals (monitored by an electroencephalograph) triggered by stimulation of sensory nerves from the skin. In this case the experimenter may be looking for a signal of microvolt amplitude against a background trace 10 times larger. If the response has a fixed latency, computer techniques can be used to average tens, hundreds, or even thousands of tracings, each initiated by the stimulus to the skin. This maneuver has the effect of amplifying the nonrandom event—i.e. the stimulus response—but of canceling the random events—the noise. For this latency-dependent extraction to work, it is obvious that the electrical signal must be of reasonably constant shape, with the same sign (negative or positive) and the same approximate duration on repeated trials.

There are, of course, a wide variety of known receptor processes in plants, even when we rule out, as semantic confusion, nonspecific biological responses to excessive stimulation—i.e. destructive agents. Detection of and response to significant environmental changes are essential attributes of any living organism. The older literature of botany and plant physiology is full of terms that inherently label receptor processes, though usually without distinguishing them from the resultant responses: phototropism, geotropism, thermotropism, etc. The easiest receptor processes to document and study have proved to be those related to light stimuli, some of which we have already noted in the

section on electrophysiology of plants.

All of these plant receptor processes display simple and predictable behavior of the first four physiologic parameters listed above; noise has generally not been analyzed, since only large signals have been examined. All of them also display two other essential characteristics: stimulus specificity and anatomical localization. Thus, light at 660 nm triggers hyperpolarization and light at 730 nm triggers depolarization (26), these changes resulting from reversible changes in form of the pigment phytochrome, which is probably embedded in the plasma membrane of the cell (28).

Another receptor system, which has been more extensively analyzed, is that mediating chemotaxis in bacteria. Motile bacteria, such as *Escherichia coli*, "swim" by means of flagella. In the absence of chemotactic agents, the bacteria move about randomly, tumbling erratically through the medium with individual flagella waving independently (29). When an attractive agent such as a sugar or amino acid is presented, the flagella amalgamate to form a "tail," the tumbling stops, and the bacteria move efficiently toward the agent. Genetic and protein-chemical data exist to show that each agent that induces chemotaxis must first react with one or two specific proteins in the bacterial wall and plasma membrane.

It is the lack of any plausible ana-

tomical substratum, rather than any single experimental fact or flaw, which—in our view—drives the final nail into the coffin for the Backster, Tompkins, and Bird view of plant “sensory perception.” Perception, communication, and emotion are properties of highly developed nervous systems (and perhaps in the near future will be properties of complex computer circuits). Although intracellular complexity on a microscale does exist in plant as well as animal cells, nowhere in the plant kingdom is there a gross anatomical structure that approximates the complexity of insect, or even worm, nervous systems, much less the mind-boggling intricacies of the cerebral cortex in higher primates.

## Reflections: Science and the force

Throughout this discussion, we have repeatedly invoked two essential operating principles of science: reproducible data collection and independent verification. Backster's conclusions, as well as many others in the Tompkins-Bird book, collapse under the test of either principle. But fortunately or unfortunately—depending on your point of view—ideas, however illogical, often prove very tenacious in men's minds. Over the past century, large numbers of scientists, perhaps even a majority, have clung to pet hypotheses long past the time when data and other scientists have laid those hypotheses to rest. Eventually, time erases (or at least blurs) our memory of such dead hypotheses, though occasionally one will be resuscitated or even reincarnated in fruitful form at a later time.

But that corpus of fallacious or unprovable claims which comprise *The Secret Life of Plants* is being kept alive in the popular literature by highly efficient mass-media techniques. The body has been kept breathing despite the fact that its brain is obviously dead. The rationale for this was given by Backster in an interview with the *Christian Science Monitor* (30):

The only problem in this kind of research is that Mother Nature does not want to jump through the hoop ten times in a row, simply because someone wants her to. It is difficult to structure repeatable experiments. There are some phenomena that occur that make this kind of thing very

difficult. For instance, once you are sure something will happen, it very well may not. I suspect that's because you are communicating to the biological material as long as you keep your consciousness involved in the experiment.

The proposition has been stated slightly differently by Marcel Vogel, another Tompkins-Bird hero (1, p. 27):

Hundreds of laboratory workers around the world are going to be frustrated and disappointed until they realize that empathy between plant and human is a key, and learn how to establish it. No amount of checking in laboratories is going to prove a thing, until the experiments are done by properly trained observers. Spiritual development is indispensable, but this runs counter to the philosophy of many scientists who do not realize that creative experimentation means that the experimenters must become part of their experiments.

This is a no-lose proposition. Negative results must be discounted because the experimenter is not “in tune,” and only positive results are accepted. The operating principles of science are set aside, and the arguments become removed from contact with physical reality. The scientific method is excluded, the questions posed quickly become irrelevant to science, and we are left in the realm of Ben Kenobi and Darth Vader.

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